

SURPRISED BY THE HOT HAND FALLACY? A TRUTH IN THE LAW OF SMALL NUMBERS

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We prove that a subtle but substantial bias exists in a common measure of the conditional dependence of present outcomes on streaks of past outcomes in sequential data. The magnitude of this *streak selection bias* generally decreases as the sequence gets longer, but increases in streak length, and remains substantial for a range of sequence lengths often used in empirical work. We observe that the canonical study in the influential hot hand fallacy literature, along with replications, are vulnerable to the bias. Upon correcting for the bias, we find that the longstanding conclusions of the canonical study are reversed.

KEYWORDS: Law of small numbers, alternation bias, negative recency bias, gambler's fallacy, hot hand fallacy, hot hand effect, sequential decision making, sequential data, selection bias, finite sample bias, small sample bias.

1. INTRODUCTION

JACK THE RESEARCHER TAKES A COIN from his pocket and decides to flip it, say, one hundred times. As he is curious about what outcome typically follows a heads, whenever he flips a heads he commits to writing down the outcome of the next flip on the scrap

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of paper next to him. Upon completing the one hundred flips, Jack of course expects the proportion of heads written on the scrap of paper to be one-half. Shockingly, Jack is wrong. For a fair coin, the expected proportion of heads is smaller than one-half.

We prove that for any finite sequence of binary data, in which each outcome of “success” or “failure” is determined by an i.i.d. random variable, the proportion of successes among the outcomes that immediately follow a streak of consecutive successes is expected to be strictly less than the underlying (conditional) probability of success.¹ While the magnitude of this *streak selection bias* generally decreases as the sequence gets longer, it increases in streak length, and remains substantial for a range of sequence lengths often used in empirical work.

We observe that the canonical study in the influential *hot hand fallacy* literature,² Gilovich, Vallone, and Tversky (1985), along with replications, have mistakenly employed a biased selection procedure that is analogous to Jack’s. Upon conducting a de-biased analysis, we find that the longstanding conclusions of the canonical study are reversed.

To illustrate how the selection procedure that Jack uses in the opening example leads to a bias, consider the simplest case in which he decides to flip the coin three times, rather than 100. In this case, there are only $2^3 = 8$ possibilities for the *single* three-flip sequence that Jack will observe. Column one of Table 1 lists these, with the respective flips that Jack would record (write down) underlined for each possible sequence. Column two gives the respective proportion of heads on recorded flips for each possible sequence. As Jack is equally likely to encounter each sequence, one can see that the expected proportion is strictly less than $1/2$, and in this case is $5/12$.³ Notice that because the sequence (rather than the flip) is the primitive outcome, the weight that the (conditional) expectation places on each sequence’s associated proportion is independent of the number of recorded flips.⁴

In Section 2, we prove the existence of the streak selection bias for the general case, then quantify it with a formula that we provide. In the case of streaks of length $k = 1$ (as in the examples discussed above), the formula admits a simple representation, and the bias is tightly related to a form of finite sample bias that shows up in autoregressive coefficient estimators (Yule (1926), Shaman and Stine (1988)).⁵ By contrast, for the more

¹The expectation is conditional on the appearance of at least one streak of k consecutive heads within the first $n - 1$ trials, where $n \geq 3$ and $1 \leq k < n - 1$.

²The hot hand fallacy has been given considerable weight as a candidate explanation for various puzzles and behavioral anomalies identified in the domains of financial markets, sports wagering, casino gambling, and lotteries (Arkes (2011), Avery and Chevalier (1999), Barberis and Thaler (2003), Brown and Sauer (1993), Camerer (1989), Croson and Sundali (2005), De Bondt (1993), Long et al. (1991), Durham, Hertz, and Martin (2005), Galbo-Jørgensen, Suetens, and Tyran (2016), Guryan and Kearney (2008), Kahneman and Riepe (1998), Lee and Smith (2002), Loh and Warachka (2012), Malkiel (2011), Narayanan and Manchanda (2012), Paul and Weinbach (2005), Rabin and Vayanos (2010), Sinkey and Logan (2013), Smith, Levere, and Kurtzman (2009), Sundali and Croson (2006), Xu and Harvey (2014), Yuan, Sun, and Siu (2014)).

³The expectation is conditional on Jack having at least one flip to record.

⁴By contrast, if Jack were instead to observe *multiple* sequences generated from the same coin, then he could weight each proportion according to its number of recorded flips when taking the average proportion across sequences. This would result in a relatively smaller bias that vanishes in the limit (see Appendix A.2).

⁵In the context of time series regression, this bias is known as the *Hurwicz bias* (Hurwicz (1950)), which is exacerbated when one introduces fixed effects into a time series model with few time periods (Nerlove (1967, 1971), Nickell (1981)). In Supplemental Material Appendix F.1 (Miller and Sanjurjo (2018)), we use a sampling-without-replacement argument to show that in the case of $k = 1$, the streak selection bias, along with finite sample bias for autocorrelation (and time series), are essentially equivalent to: (i) a form of selection bias known in the statistics literature as Berkson’s bias, or Berkson’s paradox (Berkson (1946), Roberts et al. (1978)), and (ii) several classic conditional probability puzzles.

TABLE I
THE BIAS IN THE CASE OF THREE COIN FLIPS

Three-flip sequence	Proportion of Hs on recorded flips
TTT	—
TTH	—
THT	0
HTT	0
THH	1
HTH	0
HHT	$\frac{1}{2}$
HHH	1
Expectation:	$\frac{5}{12}$

Notes: Column one lists the eight sequences that are possible for three flips of a fair coin. The proportion of heads on the flips that immediately follow one or more heads is reported in Column two, for each sequence that has at least one such flip. The (conditional) expectation of the proportion, which is simply its arithmetic average across the six equally likely sequences for which it is defined, is reported in the bottom row.

general case of $k > 1$, the streak selection bias is typically of larger magnitude, and the formula does not appear to admit a simple representation.⁶ In this case, we provide a formula for the bias that is numerically tractable for the sequence lengths commonly used in the literature that we discuss.

The bias has important implications for the analysis of streak effects in the hot hand fallacy literature. The fallacy refers to the conclusion of the seminal work of Gilovich, Vallone, and Tversky (1985; henceforth GVT), in which the authors found that despite the near ubiquitous belief among basketball fans and experts that there is momentum in shooting performance (“hot hand” or “streak” shooting), the conclusion from their statistical analyses was that momentum did not exist.⁷ The result has long been considered a surprising and stark exhibit of irrational behavior, as professional players and coaches have consistently rejected the conclusion, and its implications for their decision making. Indeed, in the years since the seminal paper was published, a consensus has emerged that the hot hand is a “myth,” and the associated belief a “massive and widespread cognitive illusion” (Thaler and Sunstein (2008), Kahneman (2011)).

We find that GVT’s critical test of hot hand shooting is vulnerable to the bias for the following simple reason: just as it is (surprisingly) incorrect to expect a fair coin flipped 100 times to yield heads half of the time on those flips that immediately follow three consecutive heads, it is incorrect to expect a consistent 50 percent (Bernoulli i.i.d.) shooter who has taken 100 shots to make half of the shots that immediately follow a streak of three hits. Thus, after first replicating the original results using GVT’s: (i) raw data, (ii) biased measures, and (iii) statistical tests, we perform a bias correction to GVT’s measures, then repeat their statistical tests. We also run some additional (unbiased) tests as robustness

⁶In Supplemental Material Appendix D, we show that the bias can be decomposed into two factors: a form of sampling-without-replacement, and a stronger bias driven by the overlapping nature of the selection procedure. In Supplemental Material Appendix F.2, we show how the bias due to the overlapping nature of the selection procedure is related to the *overlapping words paradox* (Guibas and Odlyzko (1981)).

⁷In particular, they observed that basketball shooting is “analogous to coin tossing” and “adequately described by a simple binomial model.” From this, they concluded that the belief in the hot hand was both “erroneous” and “a powerful and widely shared cognitive illusion” Gilovich, Vallone, and Tversky (1985, pp. 312–313).

checks. In contrast with GVT's results, the bias-corrected reanalysis reveals significant evidence of streak shooting, with large effect sizes.

In a brief discussion of the related literature in Section 3, we first observe that the two replications of GVT (Avugos, Bar-Eli, Ritov, and Sher (2013a), Koehler and Conley (2003)) are similarly vulnerable to the bias. We illustrate how the results of Avugos et al. (2013a), a close replication of GVT, similarly reverse when the bias is corrected for. Miller and Sanjurjo (2015b) showed that the results of Koehler and Conley (2003), which has been referred to as “an ideal situation in which to study the hot hand” (Thaler and Sunstein (2008)), reverse when an unbiased (and more powered) analysis is performed. These results in turn agree with the unbiased analyses performed on all remaining extant controlled shooting datasets in Miller and Sanjurjo (2014). Conservative estimates of hot hand effect sizes are consistently moderate to large across studies.

It follows from these results that the hot hand is not a myth, and that the associated belief is not a cognitive illusion. In addition, because researchers have: (i) accepted the null hypothesis that players have a fixed probability of success, and (ii) treated the *mere* belief in the hot hand as a cognitive illusion, the hot hand fallacy itself can be viewed as a fallacy.⁸

Finally, because the bias is subtle and (initially) surprising, even for people well-versed in probability and statistics, those unaware of it may be susceptible to being misled, or exploited.⁹ On the most basic level, it is possible that a naïve observer could be convinced that negative sequential dependence exists in an i.i.d. random process if sample size information (i.e., the number of flips that Jack records) is obscured.¹⁰ More subtly, the bias can be leveraged to manipulate people into believing that the outcomes of an unpredictable process can be predicted at rates better than chance.¹¹ Lastly, the bias can be applied in a straightforward way to construct gambling games that appear actuarially fair, but are not.¹²

⁸While our evidence reveals that belief in the hot hand is not a fallacy, it remains possible that those who believe in the hot hand hold beliefs that are too strong (or too weak), or cannot accurately detect the hot hand when it occurs. In Section 3.5, we briefly discuss existing evidence on beliefs.

⁹In informal conversations with researchers, and surveys of students, we have found a near-universal belief that the sample proportion should be equal to the underlying probability, in expectation. The conviction with which these beliefs are often held is notable, and reminiscent of the arguments that surrounded the classic Monty Hall problem (Friedman (1998), Selvin (1975), Nalebuff (1987), Vos Savant (1990)). See Miller and Sanjurjo (2015a) for more details on the connection between the selection bias, the Monty Hall problem, and other conditional probability puzzles.

¹⁰In particular, Miller and Sanjurjo (2016) showed that the bias introduced here, in conjunction with a quasi-Bayesian model of decision making under sample size neglect (Griffin and Tversky (1992), Kahneman and Tversky (1972), Benjamin, Rabin, and Raymond (2014)), provides a novel structural candidate explanation for the persistence of gambler's fallacy beliefs.

¹¹For example, suppose that a predictor observes successive realizations from a binary (or binarized) i.i.d. random process (e.g., daily stock price movements), and is evaluated according to the *success rate* of her predictions over, say, three months. If the predictor is given the freedom of *when* to predict, then she can exceed chance in her expected success rate simply by predicting a reversal whenever there is a streak of consecutive outcomes of the same kind.

¹²A simple example is to sell the following lottery ticket for \$5. A fair coin will be flipped 4 times. For each flip, the outcome will be recorded if and only if the previous flip is a heads. If the proportion of recorded heads is strictly greater than one-half, then the ticket pays \$10; if the proportion is strictly less than one-half, then the ticket pays \$0; if the proportion is exactly equal to one-half, or if no flip is immediately preceded by a heads, then a new sequence of 4 flips is generated. While, intuitively, it seems that the expected value of the lottery must be \$5, it is instead \$4.

2. THE STREAK SELECTION BIAS

Let $\mathbf{X} = \{X_i\}_{i=1}^n$ be a sequence of binary random variables, with $X_i = 1$ a “success” and $X_i = 0$ a “failure.” A natural procedure for estimating the probability of success on trial t , conditional on trial t immediately following k consecutive successes, is to first select the subset of trials that immediately follow k consecutive successes $I_k(\mathbf{X}) := \{i : \prod_{j=i-k}^{i-1} X_j = 1\} \subseteq \{k+1, \dots, n\}$, then calculate the proportion of successes on these trials.¹³ The following theorem establishes that when $\{X_i\}_{i=1}^n$ is a sequence of i.i.d. random variables, with probability of success p and fixed length n , this procedure yields a biased estimator of the conditional probability, $\mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1) \equiv p$.

THEOREM 1: *Let $\mathbf{X} = \{X_i\}_{i=1}^n$, $n \geq 3$, be a sequence of independent Bernoulli trials, each with probability of success $0 < p < 1$. Let $\hat{P}_k(\mathbf{X})$ be the proportion of successes on the subset of trials $I_k(\mathbf{X})$ that immediately follow k consecutive successes, that is, $\hat{P}_k(\mathbf{X}) := \sum_{i \in I_k(\mathbf{X})} X_i / |I_k(\mathbf{X})|$. \hat{P}_k is a biased estimator of $\mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1) \equiv p$ for all k such that $1 \leq k \leq n-2$. In particular,*

$$E[\hat{P}_k(\mathbf{X}) | I_k(\mathbf{X}) \neq \emptyset] < p. \quad (1)$$

OUTLINE OF PROOF: In the proof contained in Appendix A, we begin by showing that the conditional expectation $E[\hat{P}_k(\mathbf{X}) | I_k(\mathbf{X}) \neq \emptyset]$ is equal to the conditional probability $\mathbb{P}(X_\tau = 1 | I_k(\mathbf{X}) \neq \emptyset)$, where τ is a trial drawn (uniformly) at random from the set of selected trials $I_k(\mathbf{X})$. Next, we show that for all eligible trials $t \in I_k(\mathbf{X})$, we have that $\mathbb{P}(X_t = 1 | \tau = t, I_k(\mathbf{X}) \neq \emptyset) \leq p$, with the inequality strict for $t < n$, which implies that $\mathbb{P}(X_\tau = 1 | I_k(\mathbf{X}) \neq \emptyset) < p$. The strict inequality for $t < n$ follows from an application of Bayes’s rule. In particular, we observe that $\mathbb{P}(X_t = 1 | \tau = t, I_k(\mathbf{X}) \neq \emptyset) = \mathbb{P}(X_t = 1 | \tau = t, \prod_{i=t-k}^{t-1} X_i = 1) \propto \mathbb{P}(\tau = t | X_t = 1, \prod_{i=t-k}^{t-1} X_i = 1) \times \mathbb{P}(X_t = 1 | \prod_{i=t-k}^{t-1} X_i = 1) = \mathbb{P}(\tau = t | X_t = 1, \prod_{i=t-k}^{t-1} X_i = 1) \times p$, and then argue that $\mathbb{P}(\tau = t | X_t = 1, \prod_{i=t-k}^{t-1} X_i = 1) < \mathbb{P}(\tau = t | X_t = 0, \prod_{i=t-k}^{t-1} X_i = 1)$ for $t < n$, which guarantees that the likelihood ratio (updating factor) is less than 1, and yields $\mathbb{P}(X_t = 1 | \tau = t, \prod_{i=t-k}^{t-1} X_i = 1) < p$ for $t < n$. The intuition for why $\tau = t$ is more likely when $X_t = 0$ is the following: because the streak of ones ($\prod_{i=t-k}^{t-1} X_i = 1$) is interrupted by $X_t = 0$, the next k trials are necessarily excluded from the set $I_k(\mathbf{X})$. This means that when $X_t = 0$, there are, on average, fewer eligible trials in $I_k(\mathbf{X})$ from which to draw (relative to when $X_t = 1$), which implies that any single trial is more likely to be drawn. *Q.E.D.*

In Supplemental Material Appendix D, we show that the downward bias can be decomposed into two factors: (i) *sampling-without-replacement*: the restriction that the finite number of available successes places on the procedure for selecting trials into $I_k(\mathbf{X})$, and (ii) *streak overlap*: the additional, and stronger, restriction that the arrangement of successes and failures in the sequence places on the procedure for selecting trials into $I_k(\mathbf{X})$.

Though $\hat{P}_k(\mathbf{X})$ is biased, it is straightforward to show that it is a consistent estimator of $\mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1)$.^{14,15}

¹³In fact, this procedure yields the maximum likelihood estimate for $\mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1)$.

¹⁴See Appendix A.2 for a proof.

¹⁵It is possible to devise alternative estimators of the conditional probability that are unbiased. For example, if the researcher were instead to control the number of selected trials by repeating the experiment until

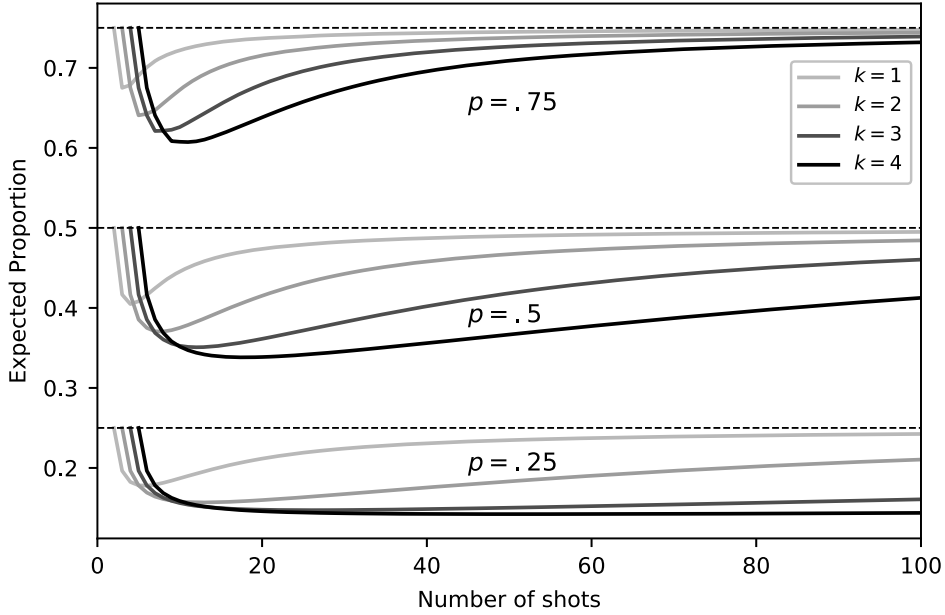


FIGURE 1.—The expected value of the proportion of successes on trials that immediately follow k consecutive successes, $\hat{P}_k(\mathbf{X})$, as a function of the total number of trials n , for different values of k and probabilities of success p , using the formula provided in Supplemental Material Appendix E.1 (Miller and Sanjurjo (2018)).

2.1. Quantifying the Bias

In Supplemental Material Appendix E.1 (Miller and Sanjurjo (2018)) we provide a formula that can be used to calculate $E[\hat{P}_k(\mathbf{X}) | I_k(\mathbf{X}) \neq \emptyset]$ for $k \geq 1$. For the special case of $k = 1$ a closed form exists, which we provide in Appendix A.3. There does not appear to be a simple representation for $k > 1$.

Figure 1 contains a plot of $E[\hat{P}_k(\mathbf{X}) | I_k(\mathbf{X}) \neq \emptyset]$, as a function of the number of trials in the sequence n , and for different values of k and p . The dotted lines in the figure represent the true probability of success for $p = .25, .50$, and $.75$, respectively. The five solid lines immediately below each dotted line represent the respective expected proportions for each value of $k = 1, 2, \dots, 5$. Observe that while the bias does generally decrease as n increases, it can remain substantial even for long sequences. For example, in the case of $n = 100$, $p = .5$, and $k = 5$, the magnitude of the bias is $.35 - .50 = -.15$, and in the case of $n = 100$, $p = .25$, and $k = 3$, the magnitude of the bias is $.16 - .25 = -.09$.¹⁶

3. APPLICATION TO THE HOT HAND FALLACY

This account explains both the formation and maintenance of the erroneous belief in the hot hand: if random sequences are perceived as streak shooting, then no amount of exposure to such sequences will convince the

he generates exactly m trials that immediately follow k consecutive successes, then the proportion would be unbiased. Alternatively, if the researcher were to eliminate the overlapping nature of the measure, there would be no bias, even though the number of selected trials would still be random.

¹⁶The non-monotonicity in n of the curves presented in Figure 1 arises because, for any streak length k , there is no bias when $n = k + 1$ (because there are only two feasible sequences, which are equally likely), or in the limit (see Appendix A.2).

player, the coach, or the fan that the sequences are in fact random. (Gilovich, Vallone, and Tversky [GVT] (1985, p. 132))

In their seminal paper, GVT found no evidence of hot hand shooting in their analysis of basketball shooting data, despite the near-unanimous belief in the hot hand among players, coaches, and fans. As a result, they concluded that belief in the hot hand is a “powerful and widely shared cognitive illusion” (p. 313).

3.1. GVT’s Analysis

Empirical Approach

GVT’s “Analysis of Conditional Probabilities” is their main test of hot hand shooting, and provides their only measure of the magnitude of the hot hand effect. The goal of their analysis is to determine whether a player’s hit probability is higher following a streak of hits than it is following a streak of misses.¹⁷ To this end, GVT reported each player i ’s shooting percentage conditional on having: (1) hit the last k shots, $\hat{P}^i(\text{hit}|k \text{ hits})$, and (2) missed the last k shots, $\hat{P}^i(\text{hit}|k \text{ misses})$, for streak lengths $k = 1, 2, 3$ (Table 4, p. 307).¹⁸ After informally comparing these shooting percentages for individual players, GVT performed a paired t -test of whether $E[\hat{P}^i(\text{hit}|k \text{ hits}) - \hat{P}^i(\text{hit}|k \text{ misses})] = 0$, for $k = 1, 2, 3$.^{19,20}

In the remainder of this section, we focus our discussion on streaks of length 3 (or more), as in, for example, Koehler and Conley (2003) and Rao (2009b), given that: (i) shorter streak lengths exacerbate attenuation bias due to measurement error (see Footnote 20 and Appendix B), and (ii) people typically perceive streaks as beginning with the third successive event (Carlson and Shu (2007)). In any case, robustness checks using different streak lengths yield similar results (see Footnotes 28 and 31 in Section 3.3).

Data

GVT analyzed shot sequences from basketball players in three contexts: NBA field goal data, NBA free-throw data, and a controlled shooting experiment with NCAA colle-

¹⁷GVT explicitly treated hot hand and streak shooting as synonymous (Gilovich, Vallone, and Tversky (1985, pp. 296–297)). Miller and Sanjurjo (2014) provided an analysis that distinguishes between hot hand and cold hand shooting, and found hot hand shooting across all extant controlled shooting data sets, but little in the way of cold hand shooting. Thus, in the present analysis, we use the terms streakiness and hot hand shooting interchangeably.

¹⁸We abuse our notation from Section 2 here in order to facilitate comparison with GVT’s analysis: we use $\hat{P}^i(\text{hit}|k \text{ hits})$ for both the random variable $\hat{P}_k(\mathbf{X})$ and its realization $\hat{P}_k(\mathbf{x})$. Similarly, we use $\hat{P}^i(\text{hit}|k \text{ misses})$ for the proportion of successes on trials that immediately follow k consecutive failures.

¹⁹Under the null hypothesis, the difference between each i ’s pair of shooting percentages is drawn from a normal distribution with mean zero.

²⁰While GVT’s analysis of conditional probabilities provides their only measure of the magnitude of the hot hand, they also analyzed the number of runs, serial correlation, and variation of shooting percentage in four-shot windows. Miller and Sanjurjo (2014) showed that the runs and serial correlation tests, along with the conditional probability test for $k = 1$, all amount to roughly the same test, and moreover, that they are not sufficiently powered to identify hot hand shooting. The reason why is due to measurement error: the act of hitting a single shot is only a weak signal of a change in a player’s underlying probability of success, which leads to an attenuation bias in the estimate of the increase in the probability of success associated with entering the hot state (see Appendix B and Stone (2012)’s work on measurement error when estimating autocorrelation in ability). The test of variation in four-shot windows is even less powered than the aforementioned tests (Wardrop (1999), Miller and Sanjurjo (2014)).

giate players. The shooting experiment was GVT's controlled test of hot hand shooting, designed for the purpose of "eliminating the effects of shot selection and defensive pressure" (p. 34), which makes it central to their main conclusions. Thus, we focus on these data below when discussing the relevance of the bias to GVT's results.^{21,22}

In GVT's controlled shooting experiment, 26 players from the Cornell University Mens' (14) and Womens' (12) basketball teams participated in an incentivized shooting task. Each player shot 100 times at a distance from which the experimenters determined he/she would make around 50 percent of the shots. Following each shot, the player had to change positions along two symmetric arcs—one facing the basket from the left, and the other from the right.

Results

In Columns 4 and 5 of Table II, we use the raw data from GVT to reproduce the shooting percentages, $\hat{P}^i(\text{hit}|3 \text{ hits})$ and $\hat{P}^i(\text{hit}|3 \text{ misses})$, for each of the 26 players (these are identical to Columns 2 and 8 of Table 4 in GVT). As indicated in GVT, players on average hit .49 when on a hit streak, versus .45 when on a miss streak. GVT's paired t -test finds the difference to be statistically indistinguishable from zero, and we replicate this result ($p = .49$).

3.2. The Bias in GVT's Analysis

While GVT's null hypothesis that $E[\hat{P}^i(\text{hit}|k \text{ hits}) - \hat{P}^i(\text{hit}|k \text{ misses})] = 0$ seems intuitively correct for a consistent shooter with a fixed probability of success p^i (i.i.d. Bernoulli), Theorem 1 reveals a flaw in this reasoning. In particular, we have established that $\hat{P}^i(\text{hit}|k \text{ hits})$ is expected to be less than p^i , and $\hat{P}^i(\text{hit}|k \text{ misses})$ greater than p^i (by symmetry). In fact, in Appendix A.4, we show that the difference $\hat{P}^i(\text{hit}|k \text{ hits}) -$

²¹From the statistical point of view, the in-game field goal data that GVT analyzed (Study 2: 76ers, 1980–81 season: 9 players, 48 home games) are not ideal for the study of hot hand shooting for reasons that are unrelated to the bias. The most notable concern with in-game field goal data is that the opposing team has incentive to make *costly* strategic adjustments to mitigate the impact of the "hot" player (Dixit and Nalebuff (1991, p. 17)). This concern has been emphasized by researchers in the hot hand literature (Aharoni and Sarig (2011), Green and Zwiebel (2017)), and is not merely theoretical, as it has a strong empirical basis. While GVT observed that a shooter's field goal percentage is lower after consecutive successes, subsequent studies have shown that with even partial controls for defensive pressure (and shot location), this effect is eliminated (Rao (2009a), Bocskocsky, Ezekowitz, and Stein (2014)). Further, evidence of specific forms of strategic adjustment has been documented (Aharoni and Sarig (2011), Bocskocsky, Ezekowitz, and Stein (2014)). See Miller and Sanjurjo (2014) for further details.

²²The in-game free throw data that GVT analyzed (Study 3: Celtics, 1980–81, 1981–82 seasons: 9 players), while arguably controlled, are not ideal for the study of hot hand shooting for a number of reasons: (i) hitting the first shot in a pair of isolated shots is not typically regarded by fans and players as hot hand shooting (Koehler and Conley (2003)), presumably due to the high prior probability of success ($\approx .75$), (ii) hitting a single shot is a weak signal of a player's underlying state, which can lead to severe measurement error (Stone (2012), Arkes (2013)), (iii) it is vulnerable to an omitted variable bias, as free throw pairs are relatively rare, and shots must be aggregated across games and seasons in order to have sufficient sample size (Miller and Sanjurjo (2014)). In any event, subsequent studies of free throw data have found evidence that is inconsistent with the conclusions that GVT drew from the Celtics' data (Wardrop (1995), Arkes (2010), Yaari and Eisenmann (2011), Aharoni and Sarig (2011), Goldman and Rao (2012), Miller and Sanjurjo (2014)).

TABLE II
SHOOTING PERFORMANCE

					$\hat{D}_3 := \hat{P}(\text{hit} 3 \text{ hits}) - \hat{P}(\text{hit} 3 \text{ misses})$	
Player	# shots	$\hat{P}(\text{hit})$	$\hat{P}(\text{hit} 3 \text{ hits})$	$\hat{P}(\text{hit} 3 \text{ misses})$	GVT est.	bias adj.
Males						
1	100	.54	.50 (12)	.44 (9)	.06	.14
2	100	.35	.00 (3)	.43 (28)	−.43	−.33
3	100	.60	.60 (25)	.67 (6)	−.07	.02
4	90	.40	.33 (3)	.47 (15)	−.13	−.03
5	100	.42	.33 (6)	.75 (12)	−.42	−.33
6	100	.57	.65 (23)	.25 (12)	.40	.48
7	75	.56	.65 (17)	.29 (7)	.36	.47
8	50	.50	.57 (7)	.50 (6)	.07	.24
9	100	.54	.83 (30)	.35 (20)	.48	.56
10	100	.60	.57 (21)	.57 (7)	.00	.09
11	100	.58	.62 (21)	.57 (7)	.05	.14
12	100	.44	.43 (7)	.41 (17)	.02	.10
13	100	.61	.50 (18)	.40 (5)	.10	.19
14	100	.59	.60 (20)	.50 (6)	.10	.19
Females						
1	100	.48	.33 (9)	.67 (9)	−.33	−.25
2	100	.34	.40 (5)	.43 (28)	−.03	.07
3	100	.39	.50 (8)	.36 (25)	.14	.23
4	100	.32	.33 (3)	.27 (30)	.07	.17
5	100	.36	.20 (5)	.22 (27)	−.02	.08
6	100	.46	.29 (7)	.54 (11)	−.26	−.18
7	100	.41	.62 (13)	.32 (25)	.30	.39
8	100	.53	.73 (15)	.67 (9)	.07	.15
9	100	.45	.50 (8)	.46 (13)	.04	.12
10	100	.46	.71 (14)	.32 (19)	.40	.48
11	100	.53	.39 (13)	.50 (10)	−.12	−.04
12	100	.25	. − (0)	.32 (37)		
Average		.47	.49	.45	.03	.13

Notes: Columns 4 and 5 reproduce the shooting percentages and number of shots that appear in Table 4, Columns 2 and 8, from Gilovich, Vallone, and Tversky (1985) (note: 3 hits (misses) includes streaks of 3, 4, 5, etc.). Column 6 reports the difference between the proportions (using the raw data), and Column 7 adjusts for the bias (mean correction), based on each player's shooting percentage (probability in this case) and number of shots.

$\hat{P}^i(\text{hit}|k \text{ misses})$ is not only expected to be negative, but that its magnitude is more than double the bias in either of the respective proportions.²³

Under GVT's design target of each player taking $n = 100$ shots and making half ($p = .5$) of them, we use the results from Section 2 and Appendix A.4 to find that the expected difference (and the strength of the bias) is -8 percentage points.²⁴ Therefore, the difference between the average proportion of $+4$ percentage points observed by GVT is actually $+12$ percentage points higher than the difference that would be expected from

²³That the difference is expected to be negative does not follow immediately from Theorem 1, as the set of sequences for which the difference is well-defined is a strict subset of the set corresponding to either of the respective proportions. Nevertheless, the reasoning of the proof is similar. See Theorem 3 of Appendix A.4.

²⁴See Figure 4 in Appendix A.4 for the bias in the difference as n , p , and k vary.

a Bernoulli i.i.d. shooter. Thus, the bias has long disguised evidence in GVT's data that may well indicate hot hand shooting.

3.3. *A Bias-Corrected Statistical Analysis of GVT*

A straightforward way to adjust for the bias in GVT's analysis is simply to shift the difference for each shooter by the amount of the corresponding bias, then repeat their paired t -test. While this test yields a statistically significant result ($p < .05$), the paired t -test limits statistical power because it reduces each player's performance to a single number, ignoring the number of shots that the player attempted in each category, that is, "3 hits" and "3 misses." In addition, adjusting for the bias based on the assumption that $p = .5$ assumes that GVT's design target was met precisely.

As a result, for each player, we again compute the bias under the null hypothesis that trials are i.i.d. Bernoulli (i.e., "consistent" shooting) but now with a probability of success equal to the player's observed shooting percentage (Column 3 of Table II), and using the number of shots taken in each category to inform our standard errors. With this approach, the average difference goes from +3 to a considerable +13 percentage points ($p < .01$, S.E. = 4.7pp).^{25,26} To put the magnitude of +13 percentage points into perspective, the difference between the median three point shooter and the top three point shooter in the 2015–2016 NBA season was 12 percentage points.²⁷ Further, this is a *conservative* estimate because, in practice, the data generating processes (i.e., shooters) clearly differ from i.i.d. Bernoulli trials, and the bias becomes much larger under various models of hot hand shooting because of measurement error (see Appendix B).

GVT also informally discussed the heterogeneity across players, and asserted that most players shot relatively better when on a streak of misses than when on a streak of hits. By contrast, Figure 2 shows that once the bias correction is made to the differences, 19 of the 25 players directionally exhibit hot hand shooting, which is itself significant ($p < .01$, binomial test).²⁸ Further, as indicated by the confidence intervals, t -tests reveal that five of the players exhibit statistically significant evidence of hot hand shooting ($p < .05$, t -test), which, for a set of 25 independent tests, is itself significant ($p < .01$, binomial test).

²⁵The standard error is computed based on the assumption of independence across the 2600 trials, and normality. In particular, defining player i 's difference $\hat{D}_k^i := \hat{P}^i(\text{hit}|k \text{ hits}) - \hat{P}^i(\text{hit}|k \text{ misses})$, the variance satisfies $\widehat{\text{Var}}(\hat{D}_k^i) = \widehat{\text{Var}}(\hat{P}^i(\text{hit}|k \text{ hits})) + \widehat{\text{Var}}(\hat{P}^i(\text{hit}|k \text{ misses}))$ for each player i . Simulations reveal that the associated $(1 - \alpha) \times 100\%$ confidence intervals with radius $z_{\alpha/2} \times \widehat{\text{Var}}(\hat{D}_k)^{1/2}$ (where the mean difference is given by $\bar{D}_k := (1/n) \sum_{i=1}^n \hat{D}_k^i$) have the appropriate coverage—that is, $(1 - \alpha/2) \times 100\%$ of the time the true difference is greater than $\bar{D}_k - z_{\alpha/2} \times \widehat{\text{Var}}(\bar{D}_k)^{1/2}$, for both Bernoulli trials and the positive feedback model discussed in Appendix B.

²⁶For an alternative approach that involves pooling shots across players, and yields similar results, see Appendix C.

²⁷ESPN, "NBA Player 3-Point Shooting Statistics—2015–16." http://www.espn.com/nba/statistics/player/_/stat/3-points [accessed September 24, 2016].

²⁸Repeating the tests for longer ($k = 4$) or shorter ($k = 2$) streak lengths yields similar results that are consistent with the attenuation bias in estimated effect sizes discussed in Footnote 20. In particular, if we instead define a streak as beginning with 4 consecutive hits, which is a stronger signal of hot hand shooting, then the average bias-adjusted difference in proportions is 10 percentage points ($p = .07$, S.E. = 6.9, one-sided test), and four players exhibit statistically significant hot hand shooting ($p < .05$), which is itself significant ($p < .01$, binomial test). On the other hand, if we define a streak as beginning with 2 consecutive hits, which is a weaker signal of hot hand shooting, then the average bias-adjusted difference in proportions is 5.4 percentage points ($p < .05$, S.E. = 3, one-sided test), and four players exhibit statistically significant hot hand shooting ($p < .05$), which is itself significant ($p < .01$, binomial test).

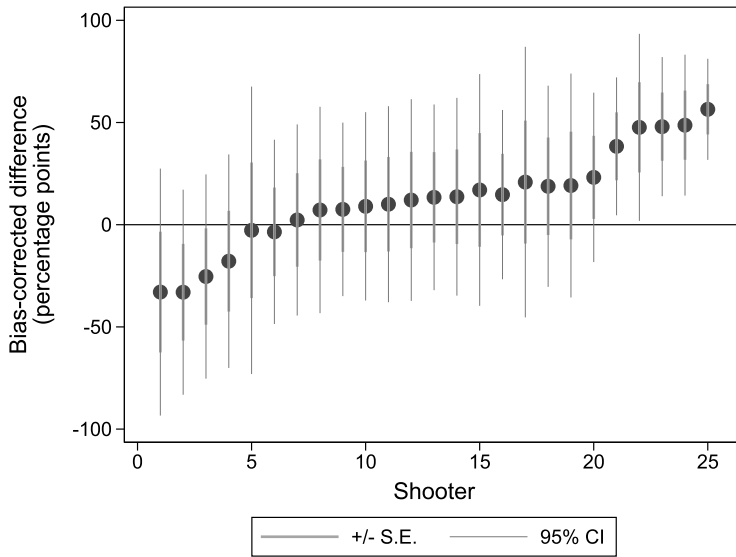


FIGURE 2.—The bias-corrected difference $\hat{D}_3^i = \hat{P}^i(\text{hit}|3 \text{ hits}) - \hat{P}^i(\text{hit}|3 \text{ misses})$ for each player, under the assumption that his/her probability of success is equal to his/her overall shooting percentage.

Nonparametric Robustness Test

As a robustness check, we perform permutation tests, which are (by construction) invulnerable to the bias. The null hypothesis for a permutation test is that a player is a consistent shooter, that is, has an i.i.d. fixed (unknown) probability of success. The first step to test for streak shooting in player i is to observe his/her shot sequence and compute the difference in proportions, $\hat{P}^i(\text{hit}|k \text{ hits}) - \hat{P}^i(\text{hit}|k \text{ misses})$. The second step is to compute this difference for each unique rearrangement of the observed sequence; each of these *permutations* is equally likely because player i 's probability of success is fixed under the null hypothesis.²⁹ The set of unique differences computed in the second step, along with their associated relative frequencies, constitutes the exact sampling distribution of the difference under the null hypothesis (conditional on the observed number of hits). This distribution can then be used for statistical testing (see Appendix C.2 for details). The distribution is negative-skewed, and can be represented by histograms such as the one shown in Figure 3, which provides the *exact* distribution for a player who has hit 50 out of 100 shots.

Results of the permutation tests agree with those of the bias-corrected tests reported above. In particular, the average difference across shooters indicates hot hand shooting with a similar level of significance ($p < .01$).³⁰ Also as before, five individual players exhibit significant hot hand shooting ($p < .01$, binomial test).³¹

²⁹Thus, the permutation procedure directly implements GVT's idea of comparing a "player's performance...to a sequence of hits and misses generated by tossing a coin" (Gilovich, Vallone, and Tversky (1985, p. 296)).

³⁰The procedure in this pooled test involves stratifying the permutations by player. In particular, we conduct a test of the average of the standardized difference, where, for each player, the difference is standardized by shifting its mean and scaling its variance under H_0 . In this case, $H_0: \mathbb{P}(\text{success on trial } t \text{ for player } i) = p^i$ for all t, i .

³¹As in Footnote 28, the results of the permutation test are robust to varying streak length k .

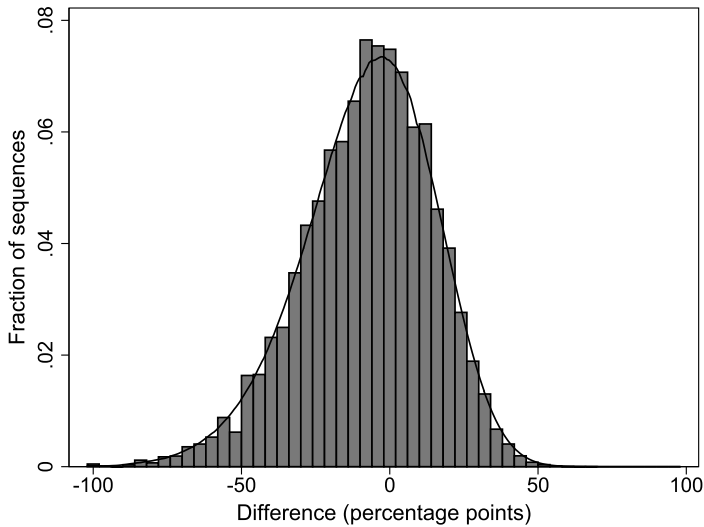


FIGURE 3.—The histogram and kernel density plot of the (exact) discrete probability distribution of $\hat{P}^i(\text{hit}|k \text{ hits}) - \hat{P}^i(\text{hit}|k \text{ misses})$, a single player i with $n = 100$ and $n_1 = 50$, using a variant of the formula for the distribution provided in Supplemental Material Appendix E.2 (Miller and Sanjurjo (2018)).

3.4. *The Hot Hand (and Bias) in Other Controlled and Semi-Controlled Studies*

A close replication of GVT's controlled shooting experiment is found in Avugos et al. (2013a), a study that mimics GVT's design and analysis, but with olympian rather than collegiate players, and fewer shots ($n = 40$) per player. From the authors' Table 1 (p. 6), one can derive the average $\hat{p}(\text{hit}|3 \text{ hits})$ and $\hat{p}(\text{hit}|3 \text{ misses})$ across players, which are roughly .52 and .54, respectively, yielding an average difference in shooting percentages of -2 percentage points.³² However, Figure 4 in Appendix A.4 shows that the strength of the bias for $n = 40$ shots and $p = .5$ (the design target) is $-.20$. Thus, once the bias is corrected for in this small sample, the average difference across shooters becomes roughly $+18$ percentage points.³³

Koehler and Conley (2003) tested for the hot hand in the NBA three point shooting contest, which has been described as an ideal setting in which to study the hot hand (Thaler and Sunstein (2008)). The authors found no evidence of hot hand shooting in their analysis of four years of data. However, as in GVT and Avugos et al. (2013a), the conditional probability tests that the authors conducted are vulnerable to the bias. By contrast, Miller and Sanjurjo (2015b) collected 28 years of data, which yield 33 players that have taken at least 100 shots; using this data set, we find that the average bias-corrected

³²We could not analyze the raw data because the authors declined to provide it to us. The data that represent a close replication of GVT are from the betting game phase. Using Table I, we have $\hat{p}(\text{hit}|3 \text{ hits}) = (.56 + .52)/2$ and $\hat{p}(\text{hit}|3 \text{ misses}) = (.54 + .49)/2$, which is the average of the shooting percentage of Group A in Phase 1 with that of Group B from Phase 2.

³³The authors also had another treatment, in which they had shooters rate, before each shot, from 0–100% on a certainty scale whether they would hit the next shot. If we repeat the analysis on the data from this treatment, then the average $\hat{p}(\text{hit}|3 \text{ hits})$ and $\hat{p}(\text{hit}|3 \text{ misses})$ across players are roughly .56 and .65, respectively, yielding an average difference of -9 percentage points, and a bias-adjusted difference of $+11$ percentage points.

difference across players is +8 percentage points ($p < .01$).³⁴ Further, 8 of the 33 players exhibit significant hot hand shooting ($p < .05$), which itself is statistically significant ($p < .001$, binomial test).

The only other controlled shooting studies that we are aware of are Jagacinski, Newell, and Isaac (1979) and Miller and Sanjurjo (2014).^{35,36} Both studies have few shooters (6 and 8, respectively) but many shots across multiple shooting sessions for each player (540 and 900+ shots, respectively). The bias-adjusted average differences in the studies are +7 and +4 percentage points, respectively. In addition, Miller and Sanjurjo (2014) found substantial and persistent evidence of hot hand shooting in individual players.³⁷

Thus, once the bias is accounted for, *conservative* estimates of hot hand effect sizes across all extant controlled and semi-controlled shooting studies are consistently moderate to large.³⁸

3.5. *Belief in the Hot Hand*

The results of our reanalysis of GVT's data lead us to a conclusion that is the opposite of theirs: belief in the hot hand is not a cognitive illusion. Nevertheless, it remains possible, perhaps even likely, that professional players and coaches sometimes infer the presence of a hot hand when it does not exist. Similarly, even when in the presence of the hot hand, players may overestimate its influence and respond too strongly to it. By contrast, a hot hand might also go undetected, or be underestimated (Stone and Arkes (2018)). These questions are important because understanding the extent to which decision makers' beliefs and behavior do not correspond to the actual degree of hot hand shooting could have considerable implications for decision-making more generally.

While GVT's main conclusion was of a binary nature, that is, based on the question of whether belief in the hot hand is either fallacious or not, they explored hot hand beliefs via a survey of player and coach beliefs, and an incentivized betting task with the Cornell players. In the survey, they found that the near-universal beliefs in the hot hand do not accord with the lack of hot hand shooting evidence that resulted from their analysis of the shooting data, and in the betting task they found that players were incapable of predicting upcoming shot outcomes successfully, which suggests that even if there were a hot hand, it could not be detected successfully.

³⁴Miller and Sanjurjo (2015b) also implemented the unbiased permutation test procedure of Section 3.3.

³⁵The one exception is a controlled shooting study that involved a single shooter: Wardrop (1999). After personal communication with the shooter, who conducted the study herself (recording her own shots), we viewed it as not having sufficient control to warrant analysis.

³⁶We thank Tom Gilovich for bringing the study of Jagacinski, Newell, and Isaac to our attention. It had gone uncited in the hot hand literature until Miller and Sanjurjo (2014).

³⁷See Avugos et al. (2013b) for a meta-analysis of the hot hand, which includes sports besides basketball. Tversky and Gilovich (1989) argued that evidence for the hot hand in other sports is not relevant to their main conclusion because so long as the hot hand does not exist in basketball, then the perception of the hot hand by fans, players, and coaches must necessarily be a cognitive illusion (see also Alter and Oppenheimer (2006)).

³⁸The magnitudes of all estimated effect sizes are conservative for two reasons: (i) if a player's probability of success is not driven merely by feedback from previous shots, but also by other time-varying player- (and environment-) specific factors, then the act of hitting consecutive shots will serve as only a noisy proxy of the hot state, resulting in measurement error, and an attenuation bias in the estimate (see Appendix B), and (ii) if the effect of consecutive successes on subsequent success is heterogeneous in magnitude (and sign) across players, then an average measure will underestimate how strong the effect can be in certain players.

However, in light of the results presented in the present paper, subjects' responses in GVT's unincentivized survey are actually qualitatively consistent with the evidence presented above.³⁹ More substantively, GVT's statistical analysis of betting data has recently been shown to be considerably underpowered, as the authors conducted many separate individual bettor level tests rather than pooling the data across bettors (Miller and Sanjurjo (2017b)). In addition, GVT misinterpreted their measures of bettors' ability to predict. In light of these limitations, Miller and Sanjurjo (2017b) reanalyzed GVT's betting data, and found that players on average shoot around +7 percentage points higher when bettors have predicted that the shot will be a hit, rather than a miss ($p < .001$). This increase is comparable in magnitude to an NBA shooter going from slightly above average to elite in three point percentage.⁴⁰

Miller and Sanjurjo (2014) presented complementary evidence on beliefs, in which semi-professional players rank their teammates' respective increases in shooting percentage when on a streak of three hits (relative to their base rates) in a shooting experiment that the rankers do not observe. Players' rankings are found to be highly correlated with their teammates' actual increases in shooting percentage in this out-of-sample test, yielding an average correlation of $-.60$ ($p < .0001$, where 1 is the rank of the shooter with the perceived largest percentage point increase).

In sum, while it remains possible that professional players' and coaches' hot hand beliefs are poorly calibrated, this claim is not clearly supported by the existing body of evidence.

4. CONCLUSION

We prove the existence of, and quantify, a novel form of selection bias that counter-intuitively arises in some particularly simple analyses of sequential data. A key implication of the bias is that the empirical approach of the canonical hot hand fallacy paper, and its replications, are incorrect. Upon correcting for the bias, we find that the data that had previously been interpreted as demonstrating that belief in the hot hand is a fallacy, instead provide substantial evidence that it is not a fallacy to believe in the hot hand.

APPENDIX A: PROOFS RELATING TO SECTION 2

A.1. Proof of Theorem 1 (Section 2)

Define $F := \{\mathbf{x} \in \{0, 1\}^n : I_k(\mathbf{x}) \neq \emptyset\}$ to be the sample space of sequences for which $\hat{P}_k(\mathbf{X})$ is well defined. The probability distribution over F is given by $\mathbb{P}(A|F) := \mathbb{P}(A \cap F)/\mathbb{P}(F)$ for $A \subseteq \{0, 1\}^n$, where $\mathbb{P}(\mathbf{X} = \mathbf{x}) = p^{\sum_{i=1}^n x_i} (1-p)^{n-\sum_{i=1}^n x_i}$.

Let the random variable X_τ represent the outcome of the randomly "drawn" trial τ , which is selected as a result of the two-stage procedure that: (i) draws a sequence \mathbf{x} at random from F , according to the distribution $\mathbb{P}(\mathbf{X} = \mathbf{x}|F)$, and (ii) draws a trial τ at random from $\{k+1, \dots, n\}$, according to the distribution $\mathbb{P}(\tau = t|\mathbf{X} = \mathbf{x})$. Let τ be a uniform draw from the trials in sequence \mathbf{X} that immediately follow k consecutive successes, that is, for $\mathbf{x} \in F$, $\mathbb{P}(\tau = t|\mathbf{X} = \mathbf{x}) = 1/|I_k(\mathbf{x})|$ for $t \in I_k(\mathbf{x})$, and $\mathbb{P}(\tau = t|\mathbf{X} = \mathbf{x}) = 0$ for $t \in I_k(\mathbf{x})^C \cap \{k+1, \dots, n\}$.⁴¹ It follows that the unconditional probability distribu-

³⁹See Appendix B of Miller and Sanjurjo (2017b) for details.

⁴⁰ESPN, "NBA Player 3-Point Shooting Statistics—2015–16." http://www.espn.com/nba/statistics/player/_/stat/3-points [accessed September 24, 2016].

⁴¹For $\mathbf{x} \in F^C$, no trial is drawn, which we can represent as $\mathbb{P}(\tau = 1|\mathbf{X} = \mathbf{x}) = 1$ (for example).

tion of τ over all trials that can possibly follow k consecutive successes is given by $\mathbb{P}(\tau = t|F) = \sum_{\mathbf{x} \in F} \mathbb{P}(\tau = t|\mathbf{X} = \mathbf{x}, F) \mathbb{P}(\mathbf{X} = \mathbf{x}|F)$, for $t \in \{k+1, \dots, n\}$. The probability that this randomly drawn trial is a success, $\mathbb{P}(X_\tau = 1|F)$, must be equal to the expected proportion, $E[\hat{P}_k(\mathbf{X})|F]$.⁴²

Note that $\mathbb{P}(X_\tau = 1|F) = \sum_{t=k+1}^n \mathbb{P}(X_t = 1|\tau = t, F) \mathbb{P}(\tau = t|F)$, and $\mathbb{P}(\tau = t|F) > 0$ for $t \in \{k+1, \dots, n\}$. Below, we demonstrate that $\mathbb{P}(X_t = 1|\tau = t, F) < p$ when $t < n$, and that $\mathbb{P}(X_t = 1|\tau = n, F) = p$, which, taken together, guarantee that $\mathbb{P}(X_\tau = 1|F) < p$.

First we observe that $\mathbb{P}(X_t = 1|\tau = t, F) = \mathbb{P}(X_t = 1|\tau = t, F_t)$, where $F_t := \{\mathbf{x} \in \{0, 1\}^n : \prod_{i=t-k}^{t-1} x_i = 1\}$. Bayes's rule then yields

$$\begin{aligned} \frac{\mathbb{P}(X_t = 1|\tau = t, F_t)}{\mathbb{P}(X_t = 0|\tau = t, F_t)} &= \frac{\mathbb{P}(\tau = t|X_t = 1, F_t) \mathbb{P}(X_t = 1|F_t)}{\mathbb{P}(\tau = t|X_t = 0, F_t) \mathbb{P}(X_t = 0|F_t)} \\ &= \frac{\mathbb{P}(\tau = t|X_t = 1, F_t)}{\mathbb{P}(\tau = t|X_t = 0, F_t)} \frac{p}{1-p}. \end{aligned}$$

Therefore, for the case of $t \in \{k+1, \dots, n-1\}$, in order to show that $\mathbb{P}(X_t = 1|\tau = t, F) = \mathbb{P}(X_t = 1|\tau = t, F_t) < p$ it suffices to show that $\mathbb{P}(\tau = t|X_t = 1, F_t) < \mathbb{P}(\tau = t|X_t = 0, F_t)$, which follows below:

$$\begin{aligned} &\mathbb{P}(\tau = t|X_t = 0, F_t) \\ &= \sum_{\mathbf{x} \in F_t: x_t=0} \mathbb{P}(\tau = t|X_t = 0, \mathbf{X} = \mathbf{x}, F_t) \mathbb{P}(\mathbf{X} = \mathbf{x}|X_t = 0, F_t) \\ &= \sum_{\mathbf{x} \in F_t: x_t=0} \mathbb{P}(\tau = t|X_t = 0, \mathbf{X}_{-t} = \mathbf{x}_{-t}, F_t) \mathbb{P}(\mathbf{X}_{-t} = \mathbf{x}_{-t}|X_t = 0, F_t) \quad (2) \\ &> \sum_{\mathbf{x} \in F_t: x_t=0} \mathbb{P}(\tau = t|X_t = 1, \mathbf{X}_{-t} = \mathbf{x}_{-t}, F_t) \mathbb{P}(\mathbf{X}_{-t} = \mathbf{x}_{-t}|X_t = 1, F_t) \quad (3) \\ &= \sum_{\mathbf{x} \in F_t: x_t=1} \mathbb{P}(\tau = t|X_t = 1, \mathbf{X} = \mathbf{x}, F_t) \mathbb{P}(\mathbf{X} = \mathbf{x}|X_t = 1, F_t) \\ &= \mathbb{P}(\tau = t|X_t = 1, F_t), \end{aligned}$$

where in (2), given \mathbf{x} , we define $\mathbf{x}_{-t} := (x_1, \dots, x_{t-1}, x_{t+1}, \dots, x_n)$. To obtain the inequality in (3), we observe that: (i) $\mathbb{P}(\mathbf{X}_{-t} = \mathbf{x}_{-t}|X_t = 0, F_t) = \mathbb{P}(\mathbf{X}_{-t} = \mathbf{x}_{-t}|X_t = 1, F_t)$ because \mathbf{X} is a sequence of i.i.d. Bernoulli trials, and (ii) $\mathbb{P}(\tau = t|X_t = 1, \mathbf{X}_{-t} = \mathbf{x}_{-t}, F_t) < \mathbb{P}(\tau = t|X_t = 0, \mathbf{X}_{-t} = \mathbf{x}_{-t}, F_t)$ because τ is drawn at random (uniformly) from the set $I_k(\mathbf{x})$, which contains at least one more element (trial $t+1$) if $x_t = 1$ rather than $x_t = 0$.

For the case of $t = n$, we follow the above steps until (3), at which point an equality now emerges as $X_n = 1$ no longer yields an additional trial from which to draw, because trial n is terminal. This implies that $\mathbb{P}(\tau = n|X_n = 1, F_n) = \mathbb{P}(\tau = n|X_n = 0, F_n)$.

⁴²The identity follows by the law of total probability, with the key observation that $\hat{P}_k(\mathbf{x}) = \sum_{t \in I_k(\mathbf{x})} x_t \cdot \frac{1}{|I_k(\mathbf{x})|} = \sum_{t=k+1}^n \mathbb{P}(X_t = 1|\tau = t, \mathbf{X} = \mathbf{x}, F) \mathbb{P}(\tau = t|\mathbf{X} = \mathbf{x}, F)$.

Taking these two facts together: (i) $\mathbb{P}(X_t = 1 | \tau = t, F) < p$, for $k + 1 \leq t < n$, and (ii) $\mathbb{P}(X_n = 1 | \tau = n, F) = p$, it immediately follows that $\mathbb{P}(X_\tau = 1 | F) < p$.⁴³ Q.E.D.

A.2. Asymptotic Unbiasedness

Proof that the Proportion is Asymptotically Unbiased

To demonstrate that $\hat{P}_k(\mathbf{X})$ is a consistent estimator of $\mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1)$, first define $Y_{k,i} := \prod_{j=i-k+1}^i X_j$ for $i \geq k$. With this, $\hat{P}_k(\mathbf{X}) = \sum_{i=k+1}^n Y_{k+1,i} / \sum_{i=k}^{n-1} Y_{k,i}$. Note that each of the respective sequences $\{Y_{k,i}\}, \{Y_{k+1,i}\}$ is asymptotically uncorrelated (k fixed). Therefore, their time averages converge to their respective means almost surely, that is, $1/(n-k) \sum_{i=k}^{n-1} Y_{k,i} \xrightarrow{\text{a.s.}} E[Y_{k,i}] = p^k$, and $1/(n-k) \sum_{i=k+1}^n Y_{k+1,i} \xrightarrow{\text{a.s.}} E[Y_{k+1,i}] = p^{k+1}$.⁴⁴ The continuous mapping theorem implies that $\hat{P}_k(\mathbf{X}) \xrightarrow{\text{a.s.}} p = \mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1)$, which in turn implies consistency. Q.E.D.

Proof that Weighted Proportions are Asymptotically Unbiased

In order to prove the assertion made in Footnote 4 that the weighted average proportion over multiple realized sequences is a consistent estimator of $\mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1)$, we first define $Y_{k,i} := \prod_{j=i-k+1}^i X_j$ for $i \geq k$, just as we did in the previous proof. Then, we note that: (i) the number of trials that follow k consecutive successes in the weighted proportion taken over T sequences is given by $\sum_{t=1}^T Z_{k,t}$, where $Z_{k,t} = \sum_{i=n(t-1)+k}^{nt-1} Y_{k,i}$, and (ii) the number of successes on these trials is given by $\sum_{t=1}^T Z_{k+1,t}$, where $Z_{k+1,t} = \sum_{i=n(t-1)+k+1}^{nt} Y_{k+1,i}$. Because $Z_{k,t}$ are i.i.d. with $E[Z_{k,t}] = (n-k)p^k$, it follows that $1/T \sum_{t=1}^T Z_{k,t} \xrightarrow{\text{a.s.}} E[Z_{k,t}] = (n-k)p^k$; similarly, $1/T \sum_{t=1}^T Z_{k+1,t} \xrightarrow{\text{a.s.}} E[Z_{k+1,t}] = (n-k)p^{k+1}$. Then, the continuous mapping theorem yields the desired consistency of the weighted proportion (after sequence T), that is, $\sum_{t=1}^T Z_{k+1,t} / \sum_{t=1}^T Z_{k,t} \xrightarrow{\text{a.s.}} p = \mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1)$. Q.E.D.

A.3. Formula for the Expected Proportion (Special Case of $k = 1$)

The following lemma shows that the expected proportion $\hat{P}_1(\mathbf{X})$, conditional on a known number of successes $N_1(\mathbf{X}) = n_1$, satisfies the sampling-without-replacement formula, which, for any given trial, is less than the probability of success $\mathbb{P}(X_i | N_1(\mathbf{X}) = n_1) = \frac{n_1}{n}$.

LEMMA 1: Let $n > 1$. Then

$$E[\hat{P}_1(\mathbf{X}) | I_1(\mathbf{X}) \neq \emptyset, N_1(\mathbf{X}) = n_1] = \frac{n_1 - 1}{n - 1} \quad (4)$$

for $0 \leq n_1 \leq n$.

⁴³Note that the proof does not require that the Bernoulli trials be identically distributed. Instead, we could allow the probability distribution to vary, with $\mathbb{P}(X_i = 1) = p^i$ for $i = 1, \dots, n$, in which case our result would be that $\mathbb{P}(X_\tau = 1 | F) < E[p_\tau | F]$.

⁴⁴See Definition 3.55 and Theorem 3.57 from White (1999).

PROOF: As in the proof of Theorem 1, let τ be drawn at random from $I_1(\mathbf{X})$, which is nonempty when $N_1(\mathbf{X}) = n_1 \geq 2$ (the result is trivial when $n_1 = 1$). In order to ease notation, we let probability $\mathbb{P}(\cdot)$ represent the conditional probability $\mathbb{P}(\cdot | N_1(\mathbf{X}) = n_1)$, which is defined over the subsets of $\{\mathbf{x} \in \{0, 1\}^n : N_1(\mathbf{x}) = n_1\}$:

$$E[\hat{P}_1(\mathbf{X}) | N_1(\mathbf{X}) = n_1, I_1(\mathbf{X}) \neq \emptyset] \\ = \mathbb{P}(X_\tau = 1) \quad (5)$$

$$= \mathbb{P}(X_\tau = 1 | \tau < n) \mathbb{P}(\tau < n) + \mathbb{P}(X_n = 1 | \tau = n) \mathbb{P}(\tau = n) \\ = \sum_{t=2}^{n-1} \mathbb{P}(X_t = 1 | \tau = t) \frac{1}{n-1} + \mathbb{P}(X_n = 1 | \tau = n) \frac{1}{n-1} \quad (6)$$

$$= \frac{n-1}{n-2} \left(\frac{n_1}{n} - \frac{1}{n-1} \right) \frac{n-2}{n-1} + \frac{n_1}{n} \frac{1}{n-1} \\ = \frac{n_1 - 1}{n-1}. \quad (7)$$

In (5), equality follows by an argument analogous to that provided in the proof of Theorem 1. In (6), equality follows from the fact that $\mathbb{P}(\tau = t) = 1/(n-1)$ for all $t \in \{2, 3, \dots, n\}$.⁴⁵ In (7), equality follows from using an application of Bayes's rule to derive $\mathbb{P}(X_t = 1 | \tau = t)$, which satisfies

$$\mathbb{P}(X_t = 1 | \tau = t) = \begin{cases} \frac{n-1}{n-2} \left(\frac{n_1}{n} - \frac{1}{n-1} \right) & \text{for } t = 2, \dots, n-1, \\ \frac{n_1}{n} & \text{for } t = n. \end{cases} \quad (8)$$

In particular,

$$\mathbb{P}(X_t = 1 | \tau = t) \\ = \frac{\mathbb{P}(\tau = t | X_{t-1} = 1, X_t = 1) \mathbb{P}(X_{t-1} = 1 | X_t = 1) \mathbb{P}(X_t = 1)}{\mathbb{P}(\tau = t)} \\ = \mathbb{P}(\tau = t | X_{t-1} = 1, X_t = 1) \frac{n_1(n_1 - 1)}{n}, \quad (9)$$

where for all t , $\mathbb{P}(X_{t-1} = 1 | X_t = 1) = (n_1 - 1)/(n - 1)$, which is the likelihood that relates to sampling-without-replacement. For $t < n$, $\mathbb{P}(\tau = t | X_{t-1} = 1, X_t = 1)$, which is the likelihood that relates to the arrangement of successes and failures, satisfies

$$\mathbb{P}(\tau = t | X_{t-1} = 1, X_t = 1) = E \left[\frac{1}{M} \middle| X_{t-1} = 1, X_t = 1 \right] \\ = \sum_{x \in \{0, 1\}} E \left[\frac{1}{M} \middle| X_{t-1} = 1, X_t = 1, X_n = x \right]$$

⁴⁵Note that $\mathbb{P}(\tau = t) = \sum_{\mathbf{x}: N_1(\mathbf{x}) = n_1} \mathbb{P}(\tau = t | \mathbf{X} = \mathbf{x}) \mathbb{P}(\mathbf{X} = \mathbf{x}) = \sum_{\mathbf{x}: N_1(\mathbf{x}) = n_1, x_{t-1} = 1} \frac{1}{n_1 - x_n} \frac{1}{\binom{n}{n_1}} = \frac{1}{\binom{n}{n_1}} \left[\binom{n-2}{n_1-1} \frac{1}{n_1} + \binom{n-2}{n_1-2} \frac{1}{n_1-1} \right] = \frac{1}{n-1}$.

$$\begin{aligned} & \times \mathbb{P}(X_n = x | X_{t-1} = 1, X_t = 1) \\ &= \frac{1}{n_1} \frac{n_0}{n-2} + \frac{1}{n_1-1} \frac{n_1-2}{n-2} \\ &= \frac{1}{n-2} \left(\frac{n_0}{n_1} + \frac{n_1-2}{n_1-1} \right), \end{aligned}$$

where $M := |I_1(\mathbf{X})|$, that is, $M = n_1 - X_n$. In the case that $t = n$, clearly $\mathbb{P}(\tau = n | X_{n-1} = 1, X_n = 1) = \frac{1}{n_1-1}$. *Q.E.D.*

Formulae for Expected Value of the Proportion

The conditional expectation in Lemma 1 can be combined with $\mathbb{P}(N_1(\mathbf{X}) = n_1 | I_1(\mathbf{X}) \neq \emptyset)$ to express the expected proportion in terms of just n and p .⁴⁶

THEOREM 2: *Let $n > 2$ and $0 < p < 1$. Then*

$$E[\hat{P}_1(\mathbf{X}) | I_1(\mathbf{X}) \neq \emptyset] = \frac{\left[p - \frac{1 - (1-p)^n}{n} \right] \frac{n}{n-1}}{1 - (1-p)^{n-1}} < p. \quad (10)$$

PROOF: We first observe that in light of Lemma 1, Equation (10) can be written as follows:

$$\begin{aligned} E[\hat{P}_1(\mathbf{X}) | I_1(\mathbf{X}) \neq \emptyset] &= E[E[\hat{P}_1(\mathbf{X}) | I_1(\mathbf{X}) \neq \emptyset, N_1(\mathbf{X}) = n_1]] \\ &= E\left[\frac{N_1(\mathbf{X}) - 1}{n-1} \middle| I_1(\mathbf{X}) \neq \emptyset\right]. \end{aligned}$$

The expected value can then be computed using the binomial distribution, which yields

$$\begin{aligned} & E\left[\frac{N_1(\mathbf{x}) - 1}{n-1} \middle| I_1(\mathbf{X}) \neq \emptyset\right] \\ &= C \sum_{n_1=1}^n p^{n_1} (1-p)^{n-n_1} \left[\binom{n}{n_1} - U(n, n_1) \right] \cdot \frac{n_1-1}{n-1} \\ &= \frac{\sum_{n_1=2}^n \binom{n}{n_1} p^{n_1} (1-p)^{n-n_1} \frac{n_1-1}{n-1}}{1 - (1-p)^n - p(1-p)^{n-1}} \\ &= \frac{\frac{1}{n-1} [(np - np(1-p)^{n-1}) - (1 - (1-p)^n - np(1-p)^{n-1})]}{1 - (1-p)^n - p(1-p)^{n-1}} \\ &= \frac{\left[p - \frac{1 - (1-p)^n}{n} \right] \frac{n}{n-1}}{1 - (1-p)^{n-1}}, \end{aligned}$$

⁴⁶In a comment written about this paper, [Rinott and Bar-Hillel \(2015\)](#) provided an alternative proof for this theorem.

where $U(n, n_1)$ is the number of sequences with n_1 successes for which the proportion is undefined, and C is the constant that normalizes the total probability to 1. The second line follows because $U_1(n, n_1) = 0$ for $n_1 > 1$, $U_1(n, 0) = U_1(n, 1) = 1$, and $C = 1/[1 - (1 - p)^n - p(1 - p)^{n-1}]$.

Finally, by letting $q := 1 - p$, it is straightforward to show that the bias in $\hat{P}_1(\mathbf{X})$ is negative:

$$\begin{aligned} E[\hat{P}_1(\mathbf{X}) - p | I_1(\mathbf{X}) \neq \emptyset] &= \frac{\left[p - \frac{1 - q^n}{n} \right] \frac{n}{n-1}}{1 - q^{n-1}} - p \\ &= \frac{(n-1)(q^{n-1} - q^n) - (q - q^n)}{(n-1)(1 - q^{n-1})} \\ &< 0. \end{aligned}$$

The inequality follows from $f(x) = q^x$ being strictly decreasing and convex, which implies that $q - q^n > (n-1)(q^{n-1} - q^n)$. Q.E.D.

A.4. Expected Difference in Proportions

Let D_k be the difference in the probability of success when comparing trials that immediately follow k consecutive successes with trials that immediately follow k consecutive failures. That is, $D_k := \mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1) - \mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} (1 - X_j) = 1)$. An estimator of D_k that is used in the hot hand fallacy literature (see Section 3) is $\hat{D}_k(\mathbf{x}) := \hat{P}_k(\mathbf{x}) - [1 - \hat{Q}_k(\mathbf{X})]$, where $\hat{Q}_k(\mathbf{X})$ is the proportion of failures on the subset of trials that immediately follow k consecutive failures, $J_k(\mathbf{X}) := \{j : \prod_{i=j-k}^{j-1} (1 - X_i) = 1\} \subseteq \{k+1, \dots, n\}$.

A.4.1. Proof of the Bias in the Difference

We extend the proof of Theorem 1 to show that $\hat{D}_k(\mathbf{X})$ is a biased estimator of D_k . Recall that $I_k(\mathbf{X})$ is the subset of trials that immediately follow k consecutive successes, that is, $I_k(\mathbf{X}) := \{i : \prod_{j=i-k}^{i-1} X_j = 1\} \subseteq \{k+1, \dots, n\}$. Analogously, let $J_k(\mathbf{X})$ be the subset of trials that immediately follow k consecutive failures, that is, $J_k(\mathbf{X}) := \{j : \prod_{i=j-k}^{j-1} (1 - X_i) = 1\} \subseteq \{k+1, \dots, n\}$.

THEOREM 3: *Let $\mathbf{X} = \{X_i\}_{i=1}^n$, $n \geq 3$, be a sequence of independent Bernoulli trials, each with probability of success $0 < p < 1$. Let $\hat{P}_k(\mathbf{X})$ be the proportion of successes on the subset of trials $I_k(\mathbf{X})$ that immediately follow k consecutive successes, and $\hat{Q}_k(\mathbf{X})$ be the proportion of failures on the subset of trials $J_k(\mathbf{X})$ that immediately follow k consecutive failures. $\hat{D}_k(\mathbf{x}) := \hat{P}_k(\mathbf{x}) - [1 - \hat{Q}_k(\mathbf{X})]$ is a biased estimator of $D_k := \mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} X_j = 1) - \mathbb{P}(X_t = 1 | \prod_{j=t-k}^{t-1} (1 - X_j) = 1) \equiv 0$ for all k such that $1 \leq k < n/2$. In particular,*

$$E[\hat{D}_k(\mathbf{X}) | I_k(\mathbf{X}) \neq \emptyset, J_k(\mathbf{X}) \neq \emptyset] < 0. \quad (11)$$

PROOF: Following the notation used in the proof of Theorem 1, let $F := \{\mathbf{x} \in \{0, 1\}^n : I_k(\mathbf{x}) \neq \emptyset\}$ and $G := \{\mathbf{x} \in \{0, 1\}^n : J_k(\mathbf{x}) \neq \emptyset\}$. We will show the following:

$$\begin{aligned} E[\hat{D}_k(\mathbf{x})|F, G] &= E[\hat{P}_k(\mathbf{X})|F, G] + E[\hat{Q}_k(\mathbf{X})|F, G] - 1 \\ &= \mathbb{P}(X_\tau = 1|F, G) + \mathbb{P}(X_\sigma = 0|F, G) - 1 \end{aligned} \quad (12)$$

$$\begin{aligned} &< p + (1 - p) - 1 \\ &= 0, \end{aligned} \quad (13)$$

where in (12), as in the proof of Theorem 1, τ is a random draw from $I_k(\mathbf{x})$ and σ is an analogous random draw from $J_k(\mathbf{x})$. In particular, we will demonstrate that the inequality in (13) holds by showing that $\mathbb{P}(X_\tau = 1|F, G) < p$, which, by symmetry, implies that $\mathbb{P}(X_\sigma = 0|F, G) < 1 - p$.

To show that $\mathbb{P}(X_\tau = 1|F, G) < p$, we use an approach similar to that presented in the proof of Theorem 1. In particular, note that $\mathbb{P}(X_\tau = 1|F, G) = \sum_{t=k+1}^n \mathbb{P}(X_t = 1|\tau = t, F, G)\mathbb{P}(\tau = t|F, G)$, and $\mathbb{P}(\tau = t|F, G) > 0$ for $t \in \{k+1, \dots, n\}$. In what follows, we demonstrate that $\mathbb{P}(X_t = 1|\tau = t, F, G) < p$ when $t < n$, and that $\mathbb{P}(X_t = 1|\tau = n, F, G) = p$, which, taken together, guarantee that $\mathbb{P}(X_\tau = 1|F, G) < p$.

First we observe that $\mathbb{P}(X_t = 1|\tau = t, F, G) = \mathbb{P}(X_t = 1|\tau = t, F_t, G)$, where $F_t := \{\mathbf{x} \in \{0, 1\}^n : \prod_{i=t-k}^{t-1} x_i = 1\}$. Bayes's rule then yields

$$\begin{aligned} \frac{\mathbb{P}(X_t = 1|\tau = t, F_t, G)}{\mathbb{P}(X_t = 0|\tau = t, F_t, G)} &= \frac{\mathbb{P}(\tau = t, G|X_t = 1, F_t) \mathbb{P}(X_t = 1|F_t)}{\mathbb{P}(\tau = t, G|X_t = 0, F_t) \mathbb{P}(X_t = 0|F_t)} \\ &= \frac{\mathbb{P}(\tau = t, G|X_t = 1, F_t)}{\mathbb{P}(\tau = t, G|X_t = 0, F_t)} \frac{p}{1 - p}. \end{aligned}$$

Therefore, for the case of $t \in \{k+1, \dots, n-1\}$, in order to show that $\mathbb{P}(X_t = 1|\tau = t, F, G) = \mathbb{P}(X_t = 1|\tau = t, F_t, G) < p$ it suffices to show that $\mathbb{P}(\tau = t, G|X_t = 1, F_t) < \mathbb{P}(\tau = t, G|X_t = 0, F_t)$, which follows below:

$$\begin{aligned} &\mathbb{P}(\tau = t, G|X_t = 0, F_t) \\ &= \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 0}} \mathbb{P}(\tau = t, \mathbf{X} = \mathbf{x}|X_t = 0, F_t) \\ &= \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 0 \\ (1, \mathbf{x}_{-t}) \in F_t \cap G}} \mathbb{P}(\tau = t, \mathbf{X} = \mathbf{x}|X_t = 0, F_t) \\ &\quad + \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 0 \\ (1, \mathbf{x}_{-t}) \notin F_t \cap G}} \mathbb{P}(\tau = t, \mathbf{X} = \mathbf{x}|X_t = 0, F_t) \\ &\geq \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 0 \\ (1, \mathbf{x}_{-t}) \in F_t \cap G}} \mathbb{P}(\tau = t, \mathbf{X} = \mathbf{x}|X_t = 0, F_t) \\ &= \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 0 \\ (1, \mathbf{x}_{-t}) \in F_t \cap G}} \mathbb{P}(\tau = t|\mathbf{X} = \mathbf{x}, X_t = 0, F_t) \mathbb{P}(\mathbf{X} = \mathbf{x}|X_t = 0, F_t) \end{aligned} \quad (14)$$

$$\begin{aligned}
&= \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 0 \\ (1, \mathbf{x}_{-t}) \in F_t \cap G}} \mathbb{P}(\tau = t | X_t = 0, \mathbf{X}_{-t} = \mathbf{x}_{-t}, F_t) \mathbb{P}(\mathbf{X}_{-t} = \mathbf{x}_{-t} | X_t = 0, F_t) \\
&> \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 0 \\ (1, \mathbf{x}_{-t}) \in F_t \cap G}} \mathbb{P}(\tau = t | X_t = 1, \mathbf{X}_{-t} = \mathbf{x}_{-t}, F_t) \mathbb{P}(\mathbf{X}_{-t} = \mathbf{x}_{-t} | X_t = 1, F_t) \quad (15) \\
&= \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 1 \\ (1, \mathbf{x}_{-t}) \in F_t \cap G}} \mathbb{P}(\tau = t | \mathbf{X} = \mathbf{x}, X_t = 1, F_t) \mathbb{P}(\mathbf{X} = \mathbf{x} | X_t = 1, F_t) \\
&= \sum_{\substack{\mathbf{x} \in F_t \cap G: \\ x_t = 1}} \mathbb{P}(\tau = t, \mathbf{X} = \mathbf{x} | X_t = 1, F_t) \\
&= \mathbb{P}(\tau = t, G | X_t = 1, F_t),
\end{aligned}$$

where in (14), given \mathbf{x} , we define $\mathbf{x}_{-t} := (x_1, \dots, x_{t-1}, x_{t+1}, \dots, x_n)$, and $(b, \mathbf{x}_{-t}) := (x_1, \dots, x_{t-1}, b, x_{t+1}, \dots, x_n)$.⁴⁷ The inequality in (15) follows for the same reason as the inequality in line (3) of Theorem 1. In particular, $\mathbb{P}(\mathbf{X}_{-t} = \mathbf{x}_{-t} | X_t = 0, F_t) = \mathbb{P}(\mathbf{X}_{-t} = \mathbf{x}_{-t} | X_t = 1, F_t)$ because \mathbf{X} is a sequence of i.i.d. Bernoulli trials, and $\mathbb{P}(\tau = t | X_t = 1, \mathbf{X}_{-t} = \mathbf{x}_{-t}, F_t) < \mathbb{P}(\tau = t | X_t = 0, \mathbf{X}_{-t} = \mathbf{x}_{-t}, F_t)$ because τ is drawn at random (uniformly) from the set $I_k(\mathbf{x})$, which contains at least one more element (trial $t + 1$) if $x_t = 1$ rather than $x_t = 0$.

For the case of $t = n$, we follow the above steps until (15), at which point an equality now emerges, as $X_n = 1$ no longer yields an additional trial from which to draw, because trial n is terminal. This implies that $\mathbb{P}(\tau = n | X_n = 1, F_n, G) = \mathbb{P}(\tau = n | X_n = 0, F_n, G)$.

Taking these two facts together: (i) $\mathbb{P}(X_t = 1 | \tau = t, F, G) < p$, for $k + 1 \leq t < n$, and (ii) $\mathbb{P}(X_n = 1 | \tau = n, F, G) = p$, it immediately follows that $\mathbb{P}(X_\tau = 1 | F, G) < p$. *Q.E.D.*

A.4.2. Formula for the Expected Difference in Proportions (Special Case of $k = 1$)

In the case of $k = 1$, the expected difference in proportions admits a simple representation that is independent of p .

THEOREM 4: Let $\hat{D}_1(\mathbf{X}) := \hat{P}_1(\mathbf{X}) - (1 - \hat{Q}_1(\mathbf{X}))$. If $n > 2$ and $0 < p < 1$, then

$$E[\hat{D}_1(\mathbf{X}) | I_1(\mathbf{X}) \neq \emptyset, J_1(\mathbf{X}) \neq \emptyset] = -\frac{1}{n-1}.$$

PROOF: The method of proof is to first show that if $n > 2$ and $1 \leq n_1 \leq n - 1$, then

$$E[\hat{D}_1(\mathbf{X}) | N_1(\mathbf{X}) = n_1, I_1(\mathbf{X}) \neq \emptyset, J_1(\mathbf{X}) \neq \emptyset] = -\frac{1}{n-1},$$

which leaves us just one step from the desired result.

First, consider the case that $1 < n_1 < n - 1$. In this case, $\hat{D}_1(\mathbf{x}) := \hat{P}_1(\mathbf{x}) - (1 - \hat{Q}_1(\mathbf{x}))$ is defined for all sequences. Therefore, by the linearity of the expectation, and applying

⁴⁷Note that the second sum will have no terms for $t \geq n - k$.

Lemma 1 to $\hat{Q}_1(\mathbf{X})$ (by symmetry), we have

$$\begin{aligned} E[\hat{D}_1(\mathbf{X})|N_1(\mathbf{X}) = n_1] &= E[\hat{P}_1(\mathbf{X})|N_1(\mathbf{X}) = n_1] - E(1 - \hat{Q}_1(\mathbf{X})|N_1(\mathbf{X}) = n_1) \\ &= \frac{n_1 - 1}{n - 1} - \left(1 - \frac{n_0 - 1}{n - 1}\right) \\ &= -\frac{1}{n - 1}. \end{aligned}$$

If $n_1 = 1$, then \hat{D}_1 is defined for all sequences that do not have a 1 in the final position; there are $n - 1$ such sequences. The sequence with the 1 in the first position yields $\hat{D}_1 = 0$, while the other $n - 2$ sequences yield $\hat{D}_1 = -1/(n - 2)$. Therefore, $E[\hat{D}_1(\mathbf{X})|N_1(\mathbf{X}) = 1, I_1(\mathbf{X}) \neq \emptyset, J_1(\mathbf{X}) \neq \emptyset] = -1/(n - 1)$.

Now consider the case of $n_1 = n - 1$. The argument for this case is analogous, with \hat{D}_1 undefined for the sequence with the zero in the last position, equal to 0 for the sequence with the zero in the first position, and equal to $-1/(n - 2)$ for all other sequences.

Finally, that the conditional expectation is independent of $N_1(\mathbf{x})$ implies that $E[\hat{D}_1(\mathbf{X})|I_1(\mathbf{X}) \neq \emptyset, J_1(\mathbf{X}) \neq \emptyset]$ is independent of p , yielding the result. *Q.E.D.*

A.4.3. Quantifying the Bias for the Difference

Figure 4 contains a plot of $E[\hat{D}_k(\mathbf{X})|I_k(\mathbf{X}) \neq \emptyset, J_k(\mathbf{X}) \neq \emptyset]$ as a function of the number of trials n , and for $k = 1, 2$, and 3. Because the bias is dependent on p when $k > 1$, the difference is plotted for various values of p . These expected differences are obtained from the formula provided in Supplemental Material Appendix E.2 (Miller and Sanjurjo (2018)). The magnitude of the bias is simply the absolute value of the expected difference.

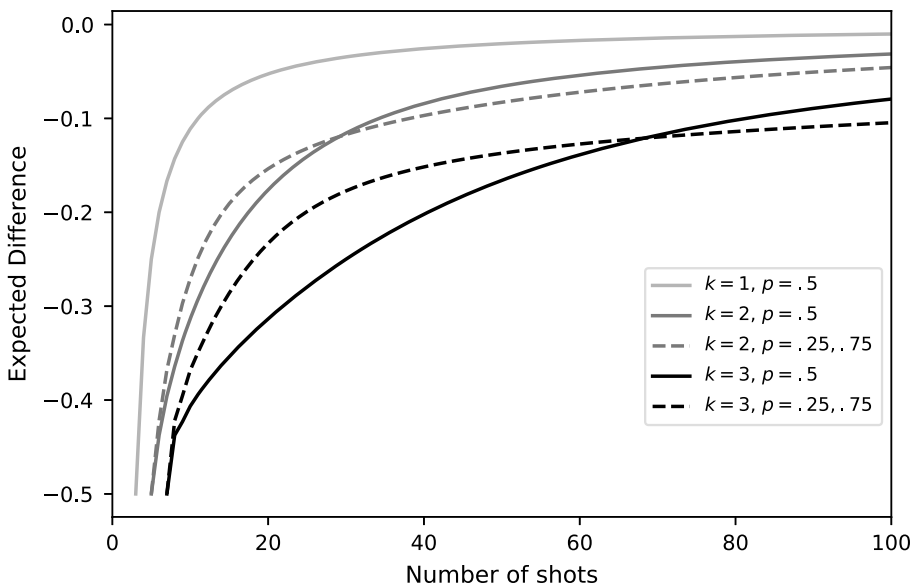


FIGURE 4.—The expected difference in the proportion of successes, as a function of n , three values of k , and various probabilities of success p , using the formula from Supplemental Material Appendix E.2.

As with the bias in the proportion (see Figure 1), the bias in the difference is substantial even when n is relatively large.

APPENDIX B

B.1. Size of the Bias When the DGP Is Hot Hand/Streak Shooting

In Section 3.3, the correction to GVT's estimate of the hot hand effect (and test statistic) is based on the magnitude of the bias under the assumption that the shooter has a fixed probability of success (Bernoulli process). However, if the underlying data generating process (DGP) instead represents hot hand or streak shooting, then the size of the bias changes. While many DGPs can produce hot hand shooting, arguably the most natural are those discussed in Gilovich, Vallone, and Tversky (1985), as they reflect lay conceptions of the hot hand and streak shooting. While GVT took no particular stance on which lay definition is most appropriate, they did identify hot hand and streak shooting with: (i) "non-stationarity" (the zone, flow, in the groove, in rhythm), and (ii) "positive association" (success breeds success). We label (i) as a *regime shift* model, and interpret it as capturing the idea that a player's probability of success may increase due to some factor that is unrelated to previous outcomes, so unobservable to the econometrician. This can be modeled naturally as a hidden Markov chain over the player's (hidden) ability state. We label (ii) as a *positive feedback* model, because it can be interpreted as capturing the idea that positive feedback from a player's previous shot outcomes can affect his/her subsequent probability of success. This can be modeled naturally as an autoregressive process, or equivalently as a Markov chain over shot outcomes.⁴⁸

In Figure 5, we plot the bias in the estimator of the difference in probability of success when on a hit streak rather than miss streak, \hat{D}_3 , for three alternative DGPs, each of which admits the Bernoulli process as a special case.⁴⁹ The first panel corresponds to the

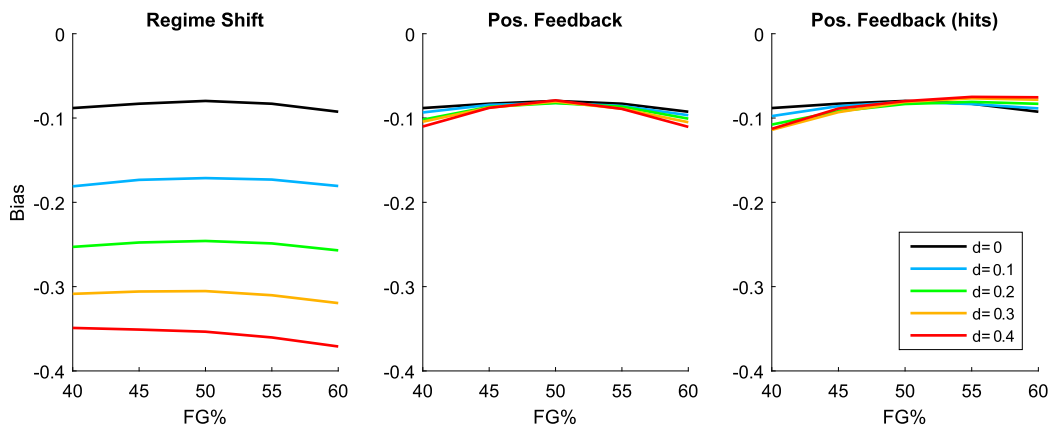


FIGURE 5.—The bias for three types of hot hand and streak shooting data generating processes (DGPs), where $FG\%$ is the expected overall field goal percentage from the DGP, and d represents the change in the player's underlying probability of success. When $d = 0$, each model reduces to a Bernoulli process. Therefore, the black line represents the bias in a Bernoulli process ($n = 100$ trials, $k = 3$).

⁴⁸A positive feedback model need not be stationary.

⁴⁹Each point is the output of a simulation with 10,000 repetitions of 100 trials from the DGP.

“regime shift” DGP in which the difference in the probability of success between the “hot” state and the “normal” state is given by d (where $d = 0$ represents Bernoulli shooting),⁵⁰ the second panel corresponds to the “positive feedback” DGP in which hitting (missing) three shots in a row increases (decreases) the probability of success by $d/2$, and the third panel corresponds to the “positive feedback (for hits)” DGP in which positive feedback operates for hits only, making the probability of success increase by d whenever three hits in a row occurs. Within each panel of the figure, the bias, which is the expected difference between \hat{D}_3 , the estimator of the shift in the probability of success, and d , the true shift in the probability of success, is depicted as a function of the expected overall shooting percentages (from 40 percent to 60 percent), for four true shifts in the underlying probability ($d \in \{.1, .2, .3, .4\}$).⁵¹

Observe that when the true DGP is a player with a hot hand, the bias is typically more severe, or far more severe, than the bias associated with a Bernoulli DGP. In particular, the bias in the “regime shift” model is particularly severe, which arises from two sources: (i) the bias discussed in Section 2, and (ii) an attenuation bias, due to measurement error, as hitting three shots in a row is an imperfect proxy for the “hot state.”⁵² The bias in the positive feedback DGP is uniformly below the bias for a Bernoulli shooter. For the DGP in which positive feedback operates only for hits, the bias is stronger than that of Bernoulli shooters for expected shooting percentages below 50 percent (as in GVT’s data), and slightly less strong for shooting percentage above 50 percent. As the true DGP is likely some combination of a regime shift and positive feedback, it is reasonable to conclude that the empirical approach in Section 3.3 should be expected to (greatly) understate the true magnitude of any underlying hot hand.

The intuition for why the introduction of regime shift elements increases the strength of the bias so considerably is that if a player’s probability of success is not driven merely by feedback from previous shots, but also by other time-varying player- (and environment-) specific factors, then the act of hitting consecutive shots will serve as only a noisy proxy of the hot state, resulting in measurement error, and an attenuation bias in the estimate. This type of measurement error is similar to what Stone (2012) uncovered in the relationship

⁵⁰In particular, let Q be the hidden Markov chain over the “normal” state (n) and the “hot” state (h), where the probability of success in the normal state is given by p_n , and the probability of success in the hot state is given by p_h , with the shift in probability of success given by $d := p_h - p_n$

$$Q := \begin{pmatrix} q_{nn} & q_{nh} \\ q_{hn} & q_{hh} \end{pmatrix},$$

where q_{nn} represents the probability of staying in the “normal” state, and q_{nh} represents the probability of transitioning from the “normal” to the “hot” state, etc. Letting $\pi = (\pi_n, \pi_h)$ be the stationary distribution, we find that the magnitude of the bias is not very sensitive to variation in the stationary distribution and transition probabilities within a plausible range (i.e., $\pi_h \in [.05, .2]$ and $q_{hh} \in (.8, .98)$), while it varies greatly with the difference in probabilities d and the overall expected shooting percentage $p = p_n + \pi_h d$. In the graph, for each d and p (FG%), we average across values for the stationary distribution (π_h) and transition probability (q_{hh}).

⁵¹Results are similar if the DGP instead has negative feedback, that is, $d \in \{-.1, -.2, -.3, -.4\}$.

⁵²In practice, observers may have more information than the econometrician (e.g., shooting mechanics, perceived confidence, or lack thereof, etc.), so may be subject to less measurement error.

between autocorrelation in outcomes and autocorrelation in ability when considering a DGP that contains autocorrelation in ability.

APPENDIX C: ADDITIONAL ANALYSES, AND DETAILS FOR SECTION 3

C.1. *An Alternative (Pooled) Analysis of Shooting Data*

An alternative approach to testing for streak shooting across players is to pool all shots from the “3 hits” and “3 misses” categories (discarding the rest), then use a linear probability model to estimate the effect of a shot falling in the “3-hits” category. If the implementation of GVT’s design met the goal of placing each player in a position in which his or her probability of success is .5, then this approach would be analogous to re-weighting the under-weighted coin flips in Table I of Section 1. With 2515 shots, the bias is minimal and the estimate in this case is +17 percentage points ($p < .01$, S.E. = 3.7). Because GVT’s design goal is difficult to implement in practice, this approach will introduce an upward bias, due to aggregation, if the probability of success varies across players. Adding fixed effects in a regression will control for this aggregation bias, but strengthens the selection bias related to streaks.⁵³ As a result, a bias correction is necessary. In this case, the estimated effect is +13.9 percentage points ($p < .01$, S.E. = 5.8), which has larger standard errors because the heteroscedasticity under the assumption of different player probabilities necessitates the use of robust variants (in this case, Bell and McCaffrey standard errors; see [Imbens and Kolesar \(2016\)](#)). The magnitude of the estimated effect has a different interpretation than the one given for the estimate of the average difference across players; it should be thought of as the hot hand effect for the average shot rather than the average player. This interpretation arises because pooling shots across players generates an unbalanced panel, which results in the estimate placing greater weight on the players that have taken more shots. As such, in the extreme it is even possible that the majority of players exhibit a tendency to have fewer streaks than expected by chance, yet, because they have generated relatively few observations, their data become diluted by many observations from a single streak shooter.

C.2. *Details on the Hypothesis Testing With the Permutation Test Procedure*

Let $\mathbf{X} \in \{0, 1\}^n$ be a sequence of shot outcomes from some player, i . The null hypothesis is that the shots are i.i.d. with $\mathbb{P}(X_t = 1) = p^i$. This implies that, conditional on the number of hits, $N_1(\mathbf{X}) = n_1$, each rearrangement is equally likely. Considering only sequences for which both $\hat{P}^i(\text{hit}|k \text{ hits})$ and $\hat{P}^i(\text{hit}|k \text{ misses})$ are defined, the hot hand hypothesis predicts that the difference $\hat{P}^i(\text{hit}|k \text{ hits}) - \hat{P}^i(\text{hit}|k \text{ misses})$ will be significantly larger than what one would expect by chance. Let $\hat{D}_k(\mathbf{X})$ be this difference for sequence \mathbf{X} . For an observed sequence \mathbf{x} , with $N_1(\mathbf{x}) = n_1$ hits, to test the null hypothesis at the α level, one simply checks if $\hat{D}_k(\mathbf{x}) \geq c_{\alpha, n_1}$, where the critical value c_{α, n_1} is defined as the smallest c such that $\mathbb{P}(D_k(\mathbf{X}) \geq c | H_0, N_1(\mathbf{X}) = n_1) \leq \alpha$, and the distribution $\mathbb{P}(D_k(\mathbf{X}) \geq c | H_0, N_1(\mathbf{X}) = n_1)$ is generated using the formula provided in Supplemental Material Appendix E.2 ([Miller and Sanjurjo \(2018\)](#)). For the quantity

⁵³In this panel regression framework, the bias from introducing fixed effects is an example of an incidental parameter problem of [Neyman and Scott \(1948\)](#), and is essentially equivalent to that discussed in [Nerlove \(1971\)](#) and [Nickell \(1981\)](#), and itself is closely related to the bias in estimates of autocorrelation in time series mentioned in the Introduction.

$\mathbb{P}(D_k(\mathbf{X}) \geq c | H_0, N_1(\mathbf{X}) = n_1)$, it may be the case that, for some c^* , it is strictly greater than α for $c \leq c^*$, and equal to 0 for $c > c^*$. In this case, for any sequence with $N_1(\mathbf{X}) = n_1$, one cannot reject H_0 at an α level of significance. From the ex ante perspective, a test of the hot hand at the α level of significance consists of a family of such critical values $\{c_{\alpha, n_1}\}$. It follows immediately that $\mathbb{P}(\text{reject} | H_0) \leq \alpha$ because $\mathbb{P}(\text{reject} | H_0) = \sum_{n_1=1}^n \mathbb{P}(D_k(\mathbf{X}) \geq c_{\alpha, n_1} | H_0, N_1(\mathbf{X}) = n_1) \mathbb{P}(N_1(\mathbf{X}) = n_1 | H_0) \leq \alpha$. Last, for any arbitrary test statistic $T(\mathbf{X})$, the fact that the distribution of \mathbf{X} is *exchangeable* conditional on $N_1(\mathbf{X}) = n_1$ means that $\mathbb{P}(T(\mathbf{X}) \geq c | H_0, N_1(\mathbf{X}) = n_1)$ can be approximated to appropriate precision with Monte Carlo permutations of the sequence \mathbf{x} .

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